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THE RISE OF THE TRANSPIRATION STREAM: AN HISTORICAL AND CRITICAL DISCUSSION.

CONTRIBUTIONS FROM THE HULL BOTANICAL LABORATORY. XXXVIII.

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[*Concluded from page 193.*]

VIII. The theories ascribing the rise of sap to the atmospheric pressure and to differences in the tension of gas within the wood are not fundamentally distinct. It is true that Böhm felt called upon to drop the former when he accepted the latter; and R. Hartig advocated the latter while altogether discrediting the other. But if the living cortex of the root be regarded as inactive, it is clear that an excess of pressure outside will force water into the root, just as surely as it will make it move in the wood toward the point where the pressure is least. This is realized in dead roots, which, as is well known (Saussure; Hansen; Janse; Böhm, 1889), can temporarily at least supply the demands of transpiration. In the living root the influence of the atmospheric pressure must be the same; it may be opposed by friction or aided by osmosis; but in itself, so long as there is water outside the root and the pressure within is less than an atmosphere, it will push the water inward. Pressing water into the root must directly or indirectly press other water up the stem. The cortex of the root is easily permeable to water; and water outside but in contact with it will be subject to and will transmit the full pressure of the atmosphere. There is, therefore, no foundation for the idea (Strasburger, 1893: 55; Goppelsroeder, 1901: 211) that the atmospheric pressure cannot operate to raise the transpiration stream merely because the walls obstruct the passage of the air itself. When root pressure is not active the atmospheric pressure is probably the chief cause of the passage of water into the root; at least this conclusion is indicated by the excellent work of

Vesque (1884), who found the absorption by oleander roots to be proportional to the pressure, both below and above one atmosphere. Vicia was found less sensitive to slight differences in the pressure, whence it appears that osmosis plays a larger part in its absorption of water.

The atmospheric pressure theory says that the water-conducting structure of a tree is something like a large water barometer. When water is withdrawn from the top of it to be evaporated, the atmospheric pressure against the bottom forces water up to make good the loss. The water column, instead of being continuous, is broken by bubbles, each supposed to have a tension corresponding to its height in the column. The tension of these bubbles will cause a local movement, and their elasticity will make any adjustment of a disturbance gradual, so that the dependence on the atmospheric pressure is not immediate, but ultimate (and entire).

The evidence for this theory, aside from Vesque's work, which has never been adequately appreciated, is chiefly the presence of rarefied air in the tracheae. We have already dwelt sufficiently on the extent of this rarefaction. The theory demands that there be an uninterrupted decrease in the tension of the air from below upward; this must hold, however, only in each individual water-path, and need not be at all true of the tree as a whole. R. Hartig (Die Gasdrucktheorie) is quoted as reckoning the difference in tension sufficient to move water between adjacent tracheae at 0.00004 atmospheres, and as making elsewhere an even lower estimate. Manometric determinations of the tension are practically valueless unless it is certain that they are made in the same path. All that has ever been proven (R. Hartig, Bot. Zeit. 1883, among others) is that the tension is on the whole less near the top of the tree, near where the water evaporates, than it is lower down. There is no sound experimental evidence that in any single water-path of any tree there is a constant increase in pressure all the way down to the roots. But on the other hand, the fact (Strasburger, 1893: 61–63) that in the crown of the tree differences in tension depend upon the intensity of the transpiration, but not on the height, is not an objection to the theory, because in the crown there must be a poor opportunity to test the tension in the same path twice; and the transverse equilibration of tensions is limited even in Tilia, where it is easiest.

The time and place of the occurrence of low tensions in trees permit no doubt as to their being the result of transpiration. They occur in the youngest wood, whence the water is drawn to the leaves; and they are greatest in the summer season of most active transpiration, in spite of (and also as a cause of) the fact that absorption by the roots is most active then. The usual daily variation is very conspicuous. In mosses with specialized conducting strands energetic transpiration results in the same way in the appearance of greatly rarefied gas (Haberlandt: 1883, and 1886: 416). Not only is it a general rule that the tension is lower during the day, but in many cases the tension exceeds an atmosphere by night, giving way to a relative suction when transpiration becomes active, as was mentioned in discussing root pressure. In our climate this occurs in summer only in herbs, but Molisch (Ann. Jard. Bot. Buit. 1898) reports it in lofty Javanese woody plants.

That the lower tension following transpiration will have as its result a forcing up of water from below until the tension is at least in part eliminated is almost self-evident. I do not recall seeing this questioned, except in one or two passages by Strasburger. After finding that branches of Taxus and Tsuga can absorb water under almost no outside pressure, he denies (1891: 796) that the tension of bubbles in the wood has any importance. And in speaking of the possibility of low tension in the upper part of a tree (1893: 60), where he says "So müsste für diese Erscheinung eine andere Ursache als die der Wasserhebung gesucht werden," I am uncertain what "Ursache" may mean. But in many places Strasburger takes the other view. For instance (1891: 871), "Die auf tracheler Saugung beruhende Wasseraufnahme aus dem Boden kann dann (after transpiration ceases to be active) fortdauern, bis dass die Bahn mit Wasser gesättigt ist," and (1893: 81) "Vor allem muss aber die negative

Gasspannung, die in solchen (zeitweise entleerten) Bahnen herrscht, zu deren Wiederfüllung beitragen. Ihr Einfluss dürfte stets dominirend sein."

That transpiration and absorption are not parallel has always been inexplicable except on the basis of a storing up of the "suction" (rather, reduced tension) by the rarefied gas in the tracheae. And this involves the assumption that the differences in tension suffice, in practice if not in theory, to lift the water.

The one great objection to this theory is that the pressure of one atmosphere at sea level is equal to that of a column of water only 10^m high, which is no considerable part of the height of large trees. The bare dogma "it cannot be" is not instructive nor stimulating, but has been much too effective in discrediting this theory. The atmospheric pressure certainly cannot cause an upward flow of a continuous column of water more than 10^m high; but except when root pressure is active and can act with the atmospheric pressure, it may well be doubted if such a column of water ever occurs in trees. The atmospheric pressure would be ineffective against it solely because its downward pressure would be greater. But it is well known that pressures in excess of an atmosphere never are found in actively transpiring trees.[1] At whatever height the pressure in the tree may be tested, it is always found such that the local application of a counter pressure of an atmosphere would move the water present upward. The problem is to explain how a pressure of one atmosphere decreases so gradually that it does not become zero in several times 10^m height of water, air, and wood. Denying the possibility has distracted attention from the real question. The conclusion of this paper will be occupied with this problem.

IX. The belief that the forces operating below—atmospheric and root pressure—and the tension of whatever kind arising from evaporation by the leaves were altogether inadequate to explain the ascent of sap left the one obvious alternative of some motive agent distributed along the stem. The only modern theories along this line making any pretence at completeness have sought this intermediate force in the activity of living cells. There are two distinct theories of this kind, proposed respectively by Westermeier and Godlewski. According to Westermeier's theory, the living cells constitute the chief path of the transpiration stream, and their osmotic activity furnishes the motive power. The vessels, and even some intercellular spaces (1884:1106–10), serve as reservoirs or landings for the water, its running down in them being prevented by capillarity. The limit on the height to which the water can well be carried at a stretch in the parenchyma alone is placed by Westermeier at 3^{cm}. It is not shown in detail that the structure of any tree is such as this theory demands; still, the association of tracheae and parenchyma is a striking phenomena, from which, in fact, Westermeier got his idea. Such rapid movement by osmosis in living cells as this theory provides for is known nowhere else.

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[1] For the explanation of Figdor's (1898) apparent discovery that high pressures are the rule in the tropics, see Molisch (1902: 53-55; also Gregor Kraus, 1895: 216). A. N. Prentiss is referred to by Clark (1874: 190) as telling of trees in Brazil which, if cut during the dry season, suck in air "so forcibly as to cause a sound, amounting almost to a groan."

Godlewski's theory is in better accord with the generally accepted fact that the tracheae are the chief path of the transpiration stream; he holding that the water passes through living cells, the medullary rays, only at occasional intervals, receiving each time an impulse like that of root pressure, which carries it some distance up in the tracheae. Godlewski's idea was that the water moved upward instead of falling in the tracheae, when forced into them by living cells, because of differences in the tension above and below; which would make this theory subject to the same limitations as if the living cell feature were omitted (Zimmermann, 1885). Janse (1887:34–48) sought to obviate this difficulty by ascribing the water's failure to sink to friction. There is no escaping the fact that water will sink in wood (unless hindered by too much air) by its own weight. For this explanation to hold it would be necessary then for the living cells to raise the water faster than it could run back, which is not to be expected of osmosis, and would be very wasteful at best.

Janse suggests that the medullary rays will always pass water in the same direction, being able to distinguish the upper end by virtue of the stem's "polarity." But the weight of evidence ever since Hales has been that the water moves with equal readiness in either direction (Hoffmann, 1850: 842; Hartig, 1861: 22; Unger, 1868: 397; MacNab, 1874: 363; and Böhm, Ber. Deut. Bot. Gesell. 1890. On the other side Rauwenhoff, Reinhold, and Meyer, 1897, the latter only *contra* Böhm's work). In view of the fact that the transpiration stream does not become more concentrated as it rises, the mechanism of its propulsion must be different from that we have found most likely in root pressure. Godlewski, therefore, embraces Pfeffer's suggestion that a stream may be driven through a cell if a difference in internal concentration be constantly maintained; and he seeks the energy to keep up this unequal distribution of solutes within the cell sap in respiration. The expensiveness to the plant of this operation is evident.

Aside from the isolated instance reported by Janse (1887: 30–32), in which the path of eosin carried by the transpiration stream seemed to traverse the medullary rays, the positive experimental evidence used to prove the activity of living stem cells is that "root" pressure is greater farther up than at the base of a stem, and that rootless parts of plants bleed. Baranetzky (Abh. Naturf. Gesell. Halle 1873: 51–53) is authority for the greater pressure higher up, but he experimented with only four individual Helianthus and Ricinus plants, too few to guarantee the results; and Detmer (1877. cf. Wieler, 1893) reached the opposite conclusion. I have found the pressure of Helianthus higher on the whole when the seedlings are cut near the ground, but have tested only a dozen plants.

Baranetzky (*l. c.* 53) also cut a piece fifteen feet long out of a maple stem, closed both ends, cut a series of wounds in it, and peeled the lower end and set it in water. After eight days the wounds, especially the lower ones, began to bleed daily, from 11 A. M. to 1:30 P. M. It was exposed to the sun from 10:30 until evening, and in spite of the fact that the temperature in the shade rose until 3 P. M., it is possible enough that its bleeding was coincident with its hours of becoming warmer. That the sap became bitter and astringent shows that its extrusion was not due to any normal activity of living cells; but the true explanation is probably the same as that of Pitra's experiments. Pitra made numerous experiments differing from Baranetzky's in that the piece of a stem was submerged except the upper end, which was provided with a manometer. Bleeding was likely to begin after eight or more days. Pitra had here substantially the same physical apparatus as an osmometer. The sap in the wood is slightly more concentrated than the water outside; between them the cambium or youngest immature wood would be a very imperfect semipermeable membrane, and the callus or healing tissue formed in and over the exposed surface within a few days (cf. Molisch, 1902; Wieler, 1893: 150) would make a much better one. A continuous layer of uninjured cells between the sap and the water would insure success; accordingly when the leafy branch was immersed and the cut end, with the manometer attached, was above the water, a pressure was more certain to be developed. This experiment shows also that in such experiments the water moves toward the base of the stem just as well as toward the leaves. Wieler (1893: 30–33) has repeated these experiments, likewise making an osmometer each time, so that his results are most naturally construed as having no bearing on the normal movement of the transpiration stream. C. Kraus has published an enormous number of instances of so-called bleeding from various isolated parts of plants In at least a part of these the conditions were such that by the formation of a wound tissue an osmometer was formed. I have not had access to all of C. Kraus' work, but what I have seen makes me quite ready to accept the opinion of Molisch that the extrusion of water was always a direct or indirect effect of wounding, and without any relation to the rise of sap in healthy plants. Molisch's own work (1899) on the voluminous bleeding of sugar producing palms was more of a puzzle in the absence of information on the histogenetic results of the wounds inflicted, until his paper of

1902 classing this with the work of Pitra, Böhm (1892), and C. Kraus. Looked at in this way, we have here a complete layer of live cells formed across the water conducting tissues, making still another osmometer[2] — an enormous nectary. The water being drawn into this nectary from the conducting elements by osmosis, it is exactly the same, so far as concerns the problem of the ascent of the water, as though it were drawn by the same force into the transpiring cells of the leaf.

Strasburger, who can speak with most authority, says the structure of many trees, notably Dracaena (1893:17), is much less favorable to the action of living cells than is that of gymnosperms, which were Godlewski's chief subject of discussion.

The most conclusive argument against the activity of living cells in the ascent of sap is its moving freely upward in stems where the cells have been killed by poisons or by boiling. More or less injurious stains have always been used to detect the path of the transpiration stream. Of more violent poisons, $K_3Fe CN_6$ was carried up in experiments for the same purpose by Rominger, Hoffmann (1848 ; 1850 : 796,842), Von Mohl (1851 : 231), and Van Tieghem (118 *seq.*) Boucherie (1840), in experiments in which the poisons must have risen more than 10^m, had various ones absorbed, especially iron pyrolignite. This was also carried up more than forty feet in an experiment already described by T. Hartig (1853 : 313), and was afterward carried downward in a reversed transpiration current in an experiment by the same author (1861 : 23). Saussure (1804) let $CuSO_4$ be absorbed, which killed the plants after a few days. Dutrochet (1837: 214) found that H_2SO_4 strong enough to discolor them would rise in stems; from which he did not fail to conclude that living cells were unnecessary to its movement. Strasburger's work on the conduction of poisons culminated with an experiment (1893 : 10–17; other work, 1891 : 607–625) in which picric acid rose 21.9^m in an oak.

Experiments in which boiled parts of plants conducted water have been made by Hansen, Weber, Janse (1887 : 21), Böhm (Bot. Centralb. 1890; 1892 II; 1893), Vesque (1891 : 583), and Strasburger (1891 : 645 *seq.*). Strasburger boiled the lower 12^m of a 15^m high Wistaria, after which water rose at least 10.8^m from the base. When part of a stem has been boiled, the living part above it usually wilts after a few days. Weber found — and Janse confirmed it — that the living part above, or both above and below, the boiled part had become impervious by the growth of tyloses, or other healing devices, such as would be used to cut off an injury arising in any other way. It is, then, the activity of the live part, and not any inactivity of the killed part which is the immediate cause of the stoppage of the transpiration stream. There is no direct evidence that any activity of living cells is necessary, even to keep the dead conducting elements in lasting proper condition.

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[2] Osmometers, so-called, are not commonly used as real "measures" of osmosis ; osmoscope would be a much better word.

It was never more difficult than it is now to say safely that any performance is not possible by living cells. But it has been most thoroughly proven that their assistance is in no way necessary in the ascent of sap; it is therefore exceedingly improbable. More than this, it is evident that if water be withdrawn from any vessel by living cells and returned to the same vessel higher up, or to any other vessel in easy communication with it, the water will run down again unless it be prevented by friction or by some force outside the living cells. Friction is not available, since water will sink in wood by its own weight; and without friction, a force which can hold it from running down can also draw it up, without any help from the living cells. The work of living cells in lifting water can be effective only when these cells form a closed layer without a leak. Root pressure is made possible by the presence of such a layer of cells. But an unbroken layer of living cells across the entire path of the transpiration stream, or even across so much of it as supplies water to a single leaf, is unheard of in the stem of any plant. It is the tracheae which are uninterrupted. It is precluded by its structure, then, that the stem of any plant should serve in the elevation of sap as the root does in setting up root pressure.

When Pfeffer (1892:258–267) attacked the problem of the rise of sap, by the method of eliminating impossibilities, Strasburger had cut out the assistance of living cells as this last resort; though Pfeffer was so reluctant to be left without any that he questioned the sufficiency of Strasburger's proof. As far as Pfeffer reached any conclusion, it is this (260): "Ohne irgend eine bestimmte Voraussetzung über die Mechanik der Wasserbewegung in den Leitbahnen, kommen wir also zu dem, übrigens auch schon aus anderen Argumentationen gezogenen Schlusse, dass das Wasser anderweitig, d. h. durch in den Leitbahnen entwickelte Kräfte, in die Höhe geschafft und somit das in solcher (! !) Weise auf höheres Niveau gehobene Wasser den aus den Gefässbündeln schöpfenden Zellen geboten wird." "Wie aber im Näheren der Betrieb zu Stande kommt, ist gänzlich unaufgeklärt."

X. Of the various theories on the ascent of the sap we have found no other to have so much in its favor, logically and empirically, as that which says the ultimate cause of the upward movement of water in the wood to replace the loss by transpiration is the pressure of the atmosphere against the water absorbed by the roots. To the sufficiency of this theory it is objected that at most the atmospheric pressure can push water up only 10^m, and this objection is not refuted, but rather better applied, when it is pointed out that the downward pressure of water actually occurring in transpiring trees of whatever height is never such that one atmosphere cannot overcome it. The kernel of the whole problem is, why is this atmospheric pressure exhausted so slowly with the ascent of the tree that, whatever height is reached, and however rapidly and forcibly water may be drawn from the wood, some pressure always remains?

Vesque (1884:195) proposes that in measuring the 10^m which the atmosphere can push up only the indices of water between the bubbles should be counted, for the bubbles have no effective weight. The suggestion is good as to water at rest; and would be valid and final as to the rising stream if the bubbles moved with the water; then our problem would have been solved by Malpighi. But the bubbles do not move. As Vesque himself was first to show, the water moves past the bubbles, without as a rule disturbing their position. In the present state of our knowledge, this seems to require that liquid water be always continuous from one index of it to the next along the wall. If the pull from the leaves is operative around the bubbles, there is no known reason why that of gravity should not be. But if it were, the total pull of the water in the stem would seem to have to be equal to that of an unbroken column of water of the same height. And no such pull can be detected.

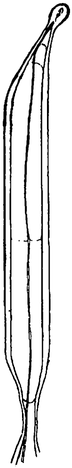
Every feature, active or structural, of the environment of the water in the wood is a part of the complex whose composite result is the rise of the water. Modifying any of these features must influence the movement of the water. Of two physiological factors — removal of the water by transpiration, and the pressure of the atmosphere — we know that in the entire absence of either the water does not rise. Any theory which leaves out of consideration any part of the environment of the water might reduce the rise of it to an equation, but would not prove its right to acceptance. The atmospheric pressure theory, taken alone, is in this negligent position. To be complete, it must reckon with the tensions, surface or other, and with the friction, external and internal, of water moving in the lumen, against the walls, and at times in the walls of the wood. Absolute completeness, such as would explain the rise of water in beech but not in oak wood, is not at present a goal; but any theory ought certainly to include every phenomenon of the rise of water in many or all trees, or show that those omitted are irrelevant.

Nothing about the rise of water strikes one who has seen it as more characteristic than its movement between the bubbles and the wall. When transpiration is at all active, the water must travel more than half of its course in this way. It must move very much more rapidly here than in places where it fills the entire lumen. Surely no theory can be complete which does not include a mechanical explanation of this passage of the bubbles.

Science has become so broad that no man can be proficient in many of its branches, and as the field of labor is divided, we turn to the physicist for skill in the solution of such purely mechanical questions as this must be. But while as keen a worker as Schwendener can find no physical ground for even the existence of a flow past bubbles, we have only ourselves to rely on for an explanation. The soundness and thoroughness of physiology is usually measured by its nearness to physics and chemistry; but in the present state of physical knowledge on this point no theory can rest on it and be acceptable. The problem is distinctly outside of my province, and the experiments I have made on it were for the personal satisfaction of feeling a new field, without any thought of clearing it. I will speak of only two or three of them.

As stated before, Vesque's experiments in glass tubes are easy to repeat. If a coiled hair is pushed into a wet tube it straightens in part, but will hold up an obstruction such as a bit of copper slid down from above; and this will afford considerable opposition to the rise of a bubble let in from below; while the hair runs down the side of a tube from the water above to that below the bubble. With care, bubbles can sometimes be held in place by the hair alone, but not as certainly. I have found it convenient to set the tube, so fitted up, in a vessel of water on the top of a microscope; the focusing adjustments of the microscope served to elevate or lower the vessel. Measurements were with a cathetometer. The meniscus in the tube being well above the obstruction to the bubble, the vessel was slowly raised; the bubble was stationary, while the meniscus higher up slowly rose. Nothing else was to be expected; this need show only that the surface tension in the angles formed by the hair and the wall is sufficient to hold in the angle water which connects that above the bubble with that below it.

In a Jamin's chain, the bubbles moving with the water, and no water passing them, capillarity will hold the highest meniscus an indefinite height up in the tube. If Vesque's suggestion, that an atmosphere should lift in the wood a total of 10^m of water, exclusive of bubbles, is tenable, the height to which the highest meniscus in a tube with a bubble past which water flows should be greater than in the same tube of unbroken water ; but at first sight that looks unlikely. Some experiments seem, however, to indicate that it occurs. Thus, in a tube 1.3^{mm} in diameter (as nearly as I could measure), the height from the water outside to the meniscus within was 23^{mm}; with two bubbles in the tube the height was 23.9^{mm}; after they were removed it was 23.1^{mm}. But in most instances I found the added height, after introducing a bubble or two, to be about 0.1^{mm}, which is too little to rely on as anything. Working with a tube obstructed by a hair alone, I have seen a bubble without any other provocation than an accidental jar, slide above the hair and make a Jamin's chain, and carry about 3^{mm} of water with it, so that the total water held up was 27.2^{mm}, instead of 24.2^{mm}. The energy to do this must have been furnished by the change in shape of the bubble. One more of these experiments will suffice. A piece of glass tubing was blown in the shape illustrated, and a bubble put in whose outline is shown by the curved lines at the two extremities of the figure ; a hair passing around the bubble. Keeping the tube upright, it was subjected to an added pressure of 70^{cm} Hg. Within ten minutes the bubble was reduced one half, as represented by the inner bounding lines ; the immediate effect of compressing the bubble was to make the meniscuses at its two ends of different shape, and this difference caused water to flow up around the bubble.



While these experiments and others show the facility with which water can pass bubbles in vessels of uneven contour, they throw no light on the force which carries it around the bubbles in the tracheae; which is the question that needs answering. The ease of the movement, and its rapidity, and above all the pressure relations involved in the tracheae, are still quite in the dark. Since the water moves farthest and fastest here, it is greatly to be hoped that physicists will soon take a position with which we can get in touch.

Another problem hardly less in the dark than the physics of the films of water around the bubbles is the resistance to be overcome by the movement of the transpiration stream. These two problems have much in common, but this one admits of a more conclusive experimental answer. According to most of the evidence we have, more energy is required to overcome this friction than in the actual lifting of the water. But this evidence is most noticeable for the discrepancies in it—cf., *e. g.* Janse with Strasburger or Sachs. In the most recent work on this subject, Curtiss (1901) concludes that the resistance offered by a stem to the flow of water at a fixed rate is not proportional to the length of stem. He also finds that the same force is required to overcome this resistance, whether it is applied as a pull or a push; which at most can be the case only within limits, because of the influence of the tension on the size of the bubbles. The rapidity of the movement of water can involve no inconvenient measure of friction, or the plant would provide for a slower current by allowing the conducting tissue a greater cross section. Instead of doing this the tree restricts its conducting area with tyloses, "gum," and changes in the wood itself, whose only known effect where they occur is to hinder the movement of water. In many trees, as in Robinia, the specialization is most decidedly toward the restriction of the transpiration stream to the youngest wood.

If it should be proven on the physical side that the water surface in contact with the bubble, in breaking half of the bonds that held it in the liquid state, had acquired some of the motility of a gas, the clearing up of the physics of the water surface would altogether alter our attitude in theory toward the friction problem. The idea that the surface of a liquid is more rigid than the interior, certainly arose from faulty experiments, but the truth of the matter is yet to be shown. If it should be found that the surface shared the properties of gases again in their relative indifferences to altitude, as Sachs suggests for the water of imbibition—and as is not at all impossible with a half-way vaporized surface—the calculation of the 10^m of water as the sum of the liquid indices alone will be justified at once. It is conceivable even that the elevation of water by surface tension in the stem be active, the source of energy being the cooling of the stem, which would remove the 10^m limitation altogether. Arendt's observations, apparently of ascending currents maintained by capillarity, have not to my knowledge been corrected or explained. It is safe to assume that the plant does not work on a dangerously narrow margin, and 10^m of water in a tree 100^m high would leave a proportion of air for which we know no parallel. But it is idle to take advantage of what we do not know to spin out speculations.

This is not a satisfactory state in which to leave a subject. But it may still be regarded as something in "this most nebulous of subjects," as Darwin calls it, to have pointed out where our theories fail, and where our densest ignorance lies. The rise of water to make good the loss by transpiration is due to the pressure of the atmosphere, or to this and other forces cooperating with it and regulated by it. Much farther than this we cannot go until there is a physical explanation of the repeated botanical observation of the passage of water between bubble and wall.

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The absence of anything like a complete and easily accessible bibliography has been a difficulty in the preparation of this paper, which I have tried to spare for future work. Not only has there been a dearth of reference and credit to earlier work, and consequently a great deal of republication, but a surprising number of inaccurate references add to the difficulty. From this list of titles I have therefore omitted all those of works which I have been unable to consult and verify in person, excepting a few which it has seemed best to refer to on hearsay; these appear in brackets. In spite of this limitation, the bibliography will hardly be found glaringly incomplete. The few general texts listed contain matter on this subject which might appropriately have been published separately. The few purely physical works included here are to complete references in the body of this paper.

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